(Of course, there need not be an infinite number of non-zero terms.) By definition, therefore, the magnitude of a given harmonic in a periodic wave is shown by the (complete) Fourier expansion.

Thus the harmonic amplitudes of a wave are unique; any indeterminacy resides completely in the choice of which *approximate reproduction* of the wave is satisfactory.

Suppose next we consider the "analysis" of a wave via the response of tuned circuits. The basic differential equation yielding the response with an arbitrary driving force can be solved by superposition. That is, since the differential equation is linear, the driving force can be represented as a sum of terms of arbitrary nature; the response is given by the sum of the separate responses to these terms. With the driving force expressed as a Fourier series (not just a polynomial approximation), the response is given as an infinite sum of harmonically related sinusoidal responses. Hence, a circuit that gives a non-zero response to only one harmonic of the fundamental frequency of the driving force will respond exactly as though only this harmonic were present in the driving force. Thus, such a filter measures the true Fourier coefficient. If we applied a *different* driving force, e.g. an approximation to the original, based on some error criterion, then the circuit would respond to the true harmonic of the *approximation.* This harmonic is, of course, related to the corresponding harmonic of the original driving force by the approximation criterion used. To sum up, the network yields the true Fourier component of the driving force actually used, whether it is really a square-wave or only an approximation thereto.

Nature has no error criterion. In nature, a square wave is always square, tautologically. Men have error criteria, to compare their approximations with their ideals. Does Nature have an ideal which she never realizes, but only approximates? Even if so, it is irrelevant, because we observe the response of circuits to their actual input signal, not their response to some hypothetical different input signal that Nature tried to apply.

CHESTER H. PAGE
National Bureau of Standards

Error-Criterion vs. Harmonic Content*

With reference to Prof. Guillemin's recent comments in this column, it is indeed true that the truncated Fourier series is not the only polynomial trigonometric approximation to a periodic function. As pointed out, the best approximating *polynomial* depends upon the criterion of approximation. Thus, in an *engineering* sense, the harmonic content of a wave that is to be synthesized can be said to depend upon the type of approximation desired in the synthesis or generation of the wave form. In the *definite* sense of harmonic, i.e. mathematical, the magnitude of a given harmonic is given by the corresponding Fourier series. This follows because the Fourier or Harmonic Analysis of a wave is purely a representation, not a God-given property of the wave. The representation of a wave as the sum of (an infinite number of) harmonics is an arbitrary convenient procedure of the mathematician and engineer. It is the *representation* that gives rise to the concept of harmonics; namely, the terms of this infinite series.

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What Is Nature's Error Criterion?*

In reply to the several responses to my blurb entitled "What is Nature's Error Criterion" I would say first of all that I am rather surprised at the intense seriousness of all these comments. As I recall the circular letter of Professor Otto J. M. Smith in which he invited any of us to send in scientific conundrums or pet peeves of some sort, the spirit was light and fluffy with a bit of humor and fun thrown in. So, when I suggested that a sleepless night might be had over this problem of mine, I hardly expected to be taken seriously. This business just goes to show what a bunch of stuffed shirts we engineers are. A guy says: "Let's have some fun"—and we start to cut each other's

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throats with mathematical analysis and limit processes. We've gotta do something about this before it's too late.

One good thing about all this, though, is the fact that this problem now will probably be better understood by more people interested in it. As all commentators have pointed out, the use of different error criteria in the determination of coefficients in the partial sum of an infinite trigonometric polynomial yields substantially different values only for the higher order terms, the lower order ones being essentially the same, regardless of the particular criterion used when the number of terms in the partial sum becomes large. Hence, as this number tends toward infinity, all finite order terms have unique values.

While this argument seems nicely to dispose of the mathematical intricacies involved, it actually only makes them less conspicuous, or covers one conundrum with another, which we all think we understand but actually don't. I refer to the word "infinite." I can comprehend the *process* of unlimited growth, but I cannot begin to understand the significance of the limit toward which such growth tends. To make myself clear on this point suppose we say: let us consider two infinite trigonometric polynomials representing the same square wave; one is a Fourier series and the other is a Féjer series. The former exhibits the Gibbs phenomenon while the latter does not, and yet all coefficients of finite order are identical. When we say "of finite order" do we imply that there are any coefficients except those of finite order? If so, which are these? Can you mention them specifically? And if not so, why bother to say "of finite order?" There is certainly a difference between the two infinite series, and yet *all* calculatable coefficients are alike.

I think it is much better to just forget about the infinite series, since we cannot understand its significance anyway. If we talk about two partial sums having an arbitrarily large number of terms, then we can clearly distinguish between the coefficients of the one that shows a tendency toward the Gibbs phenomenon and those in the one that does not.

If we were to measure harmonic amplitudes of a square wave in the laboratory, we might well ask: If we make these measurements with a tuned circuit having an arbitrarily large Q so that the response is that of a single harmonic of any finite order, will this response agree with what we calculate using Fourier analysis, in which the mean square error criterion is implicit; and if so, is there any reason based upon some physical principle why this should be so? This was the idea that I wanted to bring out by my original query.

I can easily show what the response of such a tuned circuit will be. Let the parameters be R, L, C. The damping constant is $\alpha = R/2L$; the undamped natural frequency is $\omega_0 = 1/\sqrt{LC}$; the damped natufal frequency is $\omega_d = \sqrt{\omega_0^2 - \alpha^2}$. Let the total discontinuity of the square wave be unity, and let the response just prior to the instant of a positive discontinuity be

$$i(t) = \text{Im}\,[Ae^{pt}] \text{ with } p = -\alpha + j\omega_d,$$

where Im means "imaginary part of." Then a steady state implies that after half a period, the complex amplitude of this current shall be $-A$, which gives, according to the well-known step response of this circuit,

$$\left(A + \frac{1}{L\omega_d}\right) e^{p\tau/2} = -A$$

where $\tau = 2\pi n/\omega_d$ is the period of the square wave and n is the order of the harmonic to which the circuit is tuned (an odd integer). Now

$$p^{\tau/2} = -\frac{\pi n\alpha}{\omega_d} + jn\pi,$$

so

$$e^{p\tau/2} = -e^{-\pi n\alpha/\omega d} = -1 + \frac{\pi n\alpha}{\omega_d} - \cdots$$

Since we want to evaluate the result for small damping, we get

$$A(1 - e^{-\pi n\alpha/\omega d}) = A\left(\frac{\pi n\alpha}{\omega_d} - \cdots\right)$$

$$= \frac{1 - \dfrac{\pi n\alpha}{\omega_d} + \cdots}{L\omega_d},$$

or

$$A \to \frac{1}{\pi n\alpha L} = \frac{2}{\pi n R}.$$

The Fourier coefficient for the nth harmonic is $2/\pi n$ and the impedance of the circuit when tuned to this harmonic is R. Hence this result checks with what we would calculate by Fourier methods, and so I say that the mean square error criterion is nature's error criterion. I say this because the above analysis shows that, if we let a simple linear tuned circuit (which is nature's simplest harmonic analyzer) evaluate the harmonics in a square wave (or any other wave shape if we like), then we get exactly the same ones as we do if we apply harmonic analysis with the mean square error criterion as the basis for our coefficient evaluation process.

I suspect that this result is tied in with the well-known property of linear passive circuits to respond in such a way as to make the instantaneous rate of energy dissipation a minimum, but I have not so far been able to carry such a demonstration through. I had hoped that the present discussions would yield such a correlation. Perhaps they may still do so.

E. A. Guillemin
Massachusetts Institute of Technology
Cambridge 39, Mass.